

Correspondence

The Editorial Board will be pleased to receive and consider for publication correspondence containing information of interest to physicians or commenting on issues of the day. Letters ordinarily should not exceed 600 words, and must be typewritten, double-spaced and submitted in duplicate (the original typescript and one copy). Authors will be given an opportunity to review any substantial editing or abridgement before publication.

Treatment and Prevention of Fat Embolism Syndrome

TO THE EDITOR: In reviewing the article "Fat Embolism Syndrome,"¹ I was disappointed to read the limited discussion on therapy and prevention.

In 1974 Horne and Horne, of Salt Lake City, reported on a successful program for the prophylaxis of the fat embolism syndrome.² Basically, treatment of a susceptible person with intravenously administered hypertonic glucose (with or without insulin) prevents the mobilization of fat molecules. In the ten-year interval since publication, the authors confirm that no cases of fat embolism syndrome have occurred in their practices using this prophylactic regimen despite their managing many patients with multiple large bone fractures. It would seem many others could benefit from such a successful regimen.

Unfortunately, no mention was made of this article which comes from our own western area. I hope this will bring it to the attention of those who read the *WJM* article.

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2. Horne RH, Horne JH: Fat embolization prophylaxis—Use of hypertonic glucose. *Arch Intern Med* 1974 Feb; 133:288-391

Methodology in Study of Youthful Suicides

TO THE EDITOR: I am writing regarding the article "Death in the West—A Regional Analysis of the Youthful Suicide Rate,"¹ which appeared in the June 1984 issue. Although the results are fascinating, I have several questions about the methodology:

1. On pages 969 and 970 it states, "Once the suicide incidence figures were collected for every state for each of the 15 years I calculated suicide rates using data from the 1970 US Bureau of the Census² as the population denominator." Does the author mean that he used 1970 census counts by age (15 to 24 years) for each state for each five-year period—that is, 1964-1968; 1969-1973; 1974-1978?

Unless one makes the assumption, which does not appear in the article, of linear increases in population for each state

during this 15-year period, for the western states the author has artificially lowered the rates in 1964-1968 and artificially raised them for 1974-1978.

2. The analysis is not symmetrical (or balanced) in terms of the years selected. Why did the author not group 1968-1972, giving two years around the census, rather than one year before and three years after?

3. The 1964-1968 group contains data from two to six years before the census, while the 1974-1978 group contains data from four to eight years after the census. The year groups are not equal around the census and are biased in reference to the denominator, the 1970 census.

I realize that Dr Seiden wanted to use the latest data available, 1978, but why did he not group the data differently—for example, 1963-1967; 1968-1972; 1973-1977?

4. On page 969 the author states, "Data were gathered on the incidence of suicide among 15- to 24-year-olds in each of the 50 states and the District of Columbia." However, he does not say whether these are residence or occurrence data. Although I assume he used residence data, the statement is not clear.

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2. US Census of Population 1970—Number of Inhabitants, Final Report PC (1)-A1, United States Summary. US Bureau of the Census, Government Printing Office, 1971, pp 297-309

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Dr Seiden Replies

TO THE EDITOR: With respect to the methodological questions raised by Marvin Levy, let me answer them as best I can. Questions 1 through 3 are basically variations on the question of my use of the 1970 census figures for a population denominator. I used these data as the best available population figures. The intercensal years are (sometimes rather inaccurate) estimates so I felt that I was better off using the census count itself. It is correct that I was assuming a linear increase (or decrease or balance) in state populations, but this is a reasonable assumption although it should have been made explicit. Mr Levy has answered questions 2 and 3 himself: I wanted to use the latest available data (1978), therefore the slightly asymmetrical groupings. While his critical points may affect